fleas and man. Control measures include selective dispensing of an effective insecticide dust with hand dusters into burrows, dens and nests. Sometimes, insecticide-bait stations are also set up for rodent self-application. Such flea control is the most rapid, economical and effective means to break the chain-of-infection and to protect persons using such areas. Animal extermination is not used by the state Department of Health Services or by the United States Public Health Service.

In addition to responding to outbreak situations, there is an ongoing statewide program in California for the surveillance and suppression of plague conducted by the VBCS. Whenever areas with evidence of plague activity are judged to present a risk to humans, the areas are promptly quarantined, warnings are posted and suppressive measures are undertaken with insecticides to control flea populations.

We view plague as a public health emergency. Rapid laboratory diagnosis of cases in humans is essential and, according to state regulations (Sections 2501 C and 2596, California Administrative Code, Title 17), the state Department of Health Services is to be notified immediately of all suspected cases of plague to expedite laboratory, epidemiologic, and field investigations. The identification of Yersinia pestis in the hospital laboratory by biochemical and other tests is too slow a process. Accordingly, we urge California physicians and laboratory directors to consult promptly the state's Microbial Diseases Laboratory which can quickly perform fluorescent antibody procedures, phage tests, and animal inoculations of specimen materials or suspect cultures.

The authors emphasized that "domestic (urban) plague is not a thing of the past" by pointing out that as recently as 1968, "domestic rats" in Denver were identified as infected with plague. We have no quarrel with the concept that urban plague can erupt from commingling of sylvatic and domestic rodents, but the authors should be more precise: the infected Denver rodents were not "domestic rats"—they were tree squirrels.[1] And despite the statement that an outbreak in humans was averted by a "rodent control program," a human case did occur.[1] Actually that case was identified before the epizootic in squirrels was recognized; and to control that epizootic, measures were taken against the fleas, not the squirrels. The Denver situation provides us the opportunity, however, to emphasize that with the encroachment of urban/suburban growth on previously sylvan areas, peridomestic sylvatic rodents may become as important a threat of plague transmission to man as domestic rodents were in the past.

Finally, we wish to correct for the record the statement that the fatal case of plague pneumonia in 1977 contracted by a California veterinarian resulted from his autopsy of an infected dog carcass. We investigated that case along with the Santa Clara and Monterey County Health Departments and could identify no definite source of infection: he was not known to have performed any recent postmortems, nor had he had any known flea bites or contact with animals with respiratory symptoms; he had also not camped anywhere. His clinic was in an area with a documented history of previous plague epizootic activity. While we can *speculate* that he inhaled or swallowed an infected flea (or its vomitus) from one of the cats or dogs he tended, or even a sick ground squirrel, it is far from certain that this was the source of his infection. We would prefer to report the source as unknown.[2]

S. BENSON WERNER, MD
Infectious Disease Section

RONALD R. ROBERTO, MD
Infectious Disease Section

BERNARD C. NELSON, PHD
Vector Biology and Control Section

MARJORIE BISSETT, PHD
Microbial Diseases Laboratory
California Department of Health Services
Berkeley, California

REFERENCES

1. Hudson BW, Goldenberg MI, McCluskie JD, et al: Serological and bacteriological investigations of an outbreak of plague in an urban tree squirrel population. Am J Trop Med & Hyg 20: 255-263, 1971

2. California Department of Health Services: Death from plague in a California veterinarian. California Morbidity Weekly Rep No 33, Aug 26, 1977

Cardiopulmonary Resuscitation in Patients Over 65

TO THE EDITOR: I believe the impact of cardiopulmonary resuscitation (CPR) on patients over 65 is often maleficent.

May I comment on Dr. Richard L. Coskey's careful report in the December 1978 issue (Cardiopulmonary Resuscitation: Impact on Hospital Mortality—A Ten-Year Study)? In older persons, it is my opinion that when more than five minutes is required to reestablish a viable heart rate and rhythm, the impact of CPR is more often harmful than helpful. Table 1 of Dr. Coskey's article seems to substantiate my conclusions. It reveals

that for 1973 and 1974, resuscitation was carried out in 165 patients of all ages. Even in this younger (on the average) group "increasing instrumentation" *prolonged the act of dying for 57 persons and left 16 others to exist with brain damage.*

I have studied 46 consecutive "successful" resuscitations *in patients over 65* at Peninsula Hospital and Medical Center, Burlingame, California. Of the patients, 63 percent died of their underlying disease before they left the hospital. In other words, their act of dying was prolonged.

My personal survey of internists over 65 reveals that 50 percent, in the event they suffer an instantly observed and treated "arrest," would disapprove of CPR lasting more than five minutes for themselves. If they so desire, means are available to let our sapient patients in this age group make the same informed decision. A modified living will[1] can do just this.

I would be greatly interested in Dr. Coskey's figures for his patients over 65. I hope they are not as melancholy as those from three Nurnberg hospitals who reported in 1976 that 239 patients over 60 were resuscitated "successfully." At the end of six months, only nine were alive.[2]

I believe many patients would consider this impact on their lives as summun malum.

LOUIS SHATTUCK BAER, MD
Burlingame, California

REFERENCES

1. Baer LS: Let the Patient Decide. Philadelphia, Westminster Press, 1978, pp 144-145
2. Füsgen I, Summa JD: How much sense is there in an attempt to resuscitate an aged person? Gerontology 24:37-45, 1978

* * *

Dr. Coskey Replies

TO THE EDITOR: Dr. Baer's letter reflects concern regarding possible brain damage in a patient who has otherwise undergone successful cardiopulmonary resuscitation (CPR). In our study 4 percent of all patients resuscitated had brain damage (as was shown in Table 1).[1] Patients 60 years of age or older, a total of 776 in number or 67 percent of the total 1,155 patients, had the same 4 percent incidence of brain damage as did the total group.

No time limit should be placed on CPR.[2,3] A four- to six-minute time interval might be considered if it were known that CPR was not appropriately applied and there was inadequate maintenance of cerebral blood flow. With correct application of CPR, adequate cerebral perfusion can be maintained for prolonged periods of time to prevent brain damage.

It is important in reviewing the 1973 and 1974 statistics (Table 1 of the article in the December issue) to point out that 92 patients *did not die* even though a lesser number did succumb to their underlying illness before hospital discharge. Few patients with brain damage live to see hospital discharge.[4,5]

The incidence of survival following CPR is inversely related to age. Nevertheless, 20 percent of 156 patients 60 years of age or older in this ten-year study were long-term survivors.

The data from the three Nurnberg hospitals[6] were indeed melancholy. Seven percent of the total group of 335 patients, or 24, were discharged from the hospital. Their data base is slightly different in that they only tabulated patients whose resuscitation lasted at least four minutes. Nevertheless, I would hope that, with experience and time, improvement will be noted, as was portrayed in Figure 1.

RICHARD L. COSKEY, MD
Tarzana, California

REFERENCES

1. Coskey RL: Cardiopulmonary resuscitation: Impact on hospital mortality—A ten-year study (Health Care Delivery). West J Med 129:511-517, Dec 1978
2. American Heart Association Committee on Cardiopulmonary Resuscitation and Emergency Cardiac Care: Standards for cardiopulmonary resuscitation (CPR) and emergency cardiac care (ECC). JAMA 227:833-848, 1974
3. Cardiopulmonary Resuscitation. The American National Red Cross. 1974, pp 1-41
4. Coskey RL: A resuscitation program in a community hospital—Five-year experience. Geriatrics 26:66-72, 1971
5. Stiles QR, Tucker BL, Meyer BW, et al: Cardiopulmonary arrest. Am J Surg 122:181-187, 1971
6. Füsgen I, Summa JD: How much sense is there in an attempt to resuscitate an aged person? Gerontology 24:37-45, 1978

Sex Education for Teenagers

TO THE EDITOR: The views of the Special Editor for Idaho, E. R. W. Fox, MD, in the November issue, "Sex Education for Teenagers," were most provocative. Dr. Fox suggests that physicians have been reluctant to advocate "family planning and sex education" (translation: contraception counseling) for teenagers for various and sundry reasons. Among these: because we "feared rebuff," "felt unqualified for lack of learning," "were not comfortable in dealing with this sensitive subject." I would challenge his explanations, and suggest that physicians have considerably more reason and justification for their reluctance than his essay would suggest.

Dr. Fox fails to consider, at all, the fact that many physicians may have "reneged" on their alleged "responsibilities" because they are really quite convinced that *this* approach is *itself* quite irresponsible. In fact, they may feel that his approach will do more harm than good, in the sense